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# THE SEX RATIO IN THE DOMESTIC FOWL.<sup>1</sup>

By RAYMOND PEARL.

*(Read April 13, 1917.)*

## I. INTRODUCTION.

One of the most notable biological discoveries of recent years is that which has demonstrated the cytological mechanism of sex determination. As a result of the work of McClung, Wilson, Stevens, Montgomery, Morgan, and many other investigators, we have a tolerably clear understanding of the cellular mechanism by which it is determined, in a wide variety of forms, that particular individuals are males while others are females. At first sight it would appear that the discoveries referred to had made superfluous further studies of sex ratios. The whole history of the statistical investigation of sex ratios, viewed from the standpoint of present knowledge of the mechanism of sex determination, seems a rather futile and blind groping after something which very successfully eluded that form of pursuit.

But there are still reasons, as it seems to the writer, why it is desirable to carry on certain sorts of statistical investigations of sex ratios. The most important of these is that there is a considerable body of evidence in the literature, which would seem to show, if

<sup>1</sup> Papers from the Biological Laboratory of the Maine Agricultural Experiment Station. No. 119. This paper constitutes No. VIII. of a series of "Sex Studies" by the present writer.

It was originally intended that this should be a much more extended paper than it now is. When it was presented before the Society a number of matters were discussed which do not appear here at all. This condition of affairs arises from the fact that in the midst of the preparation of the final manuscript for the printer the writer was called to war work which made impossible the completion of the paper in the form originally contemplated. In view of the impossibility of foretelling when the writing could be completed it seemed desirable to publish the portion already done rather than to leave the whole till the somewhat uncertain time of the end of the war.

taken at its face value, that sex ratios may, in some cases at least, be experimentally modified and in some degree controlled. The critical value of all of this evidence is not equal. In some instances it appears certain, and in more cases probable, that the data presented do not warrant the conclusion that the sex ratio has been either modified or controlled. There is, of course, no theoretical impossibility in modifying the sex ratio in an organism where the chromosomal mechanism of sex determination is a definite and constant one. We know of no hereditary character which may not, upon occasion, be modified; and in the case of sex the brilliant researches of Goldschmidt<sup>2</sup> make it clear that not only the somatic manifestation of the chromosomal sex mechanism may vary and be experimentally modified, but presumably also the mechanism itself. But just because of the usual and normal stability of germinal mechanisms it becomes the more important to be sure, on the one hand, that evidence alleged to demonstrate that sex ratios may be modified or controlled is sound and adequate when subjected to the scrutiny of modern statistical science, and, on the other hand, to learn more than we now know about the normal variability of sex ratios. As a contribution in this direction it seems important where possible to present and critically analyze statistical data, of adequate amount, regarding the normal sex ratio of forms frequently used in experimental work.

It is the purpose of this paper to present and analyze such data for the domestic fowl. The statistics here used cover eight years in point of time, and represent over 22,000 individual chicks.

The specific topics which will be discussed are these:

1. The normal, average sex ratio in the domestic fowl.
2. The variation in the sex ratio.
3. The influence of prenatal mortality on the sex ratio.

## II. MATERIAL AND METHODS.

Before undertaking the presentation and discussion of the statistics it is desirable to say a word in regard to their collection and analysis. The data are those which have arisen in the writer's ex-

<sup>2</sup> Goldschmidt, R., *Amer. Nat.*, 1916, and other papers.

perimental breeding operations with poultry at the Maine Agricultural Experiment Station during the breeding seasons of 1908 to 1915 inclusive. The 1916 matings are not included except for the discussion of certain special problems because the original record-taking on that year's birds is not completed at the time of writing. During the period covered by the statistics the sex of every chick which hatched was determined if it was physically possible to make such determination. Failure to determine the sex in individual cases resulted from one or another of the following kinds of causes: (*a*) The loss of the bird from predaceous enemies, thieves, or straying; (*b*) the bird's total destruction by fire; (*c*) the loss of its identifying leg band, which rendered its assignment to the proper mating impossible. In the case of birds which died before reaching an age where the development of secondary sex characters made it possible to distinguish the sexes externally, dissection and examination of the gonads was resorted to for the determination.

The number of cases of birds not sexed at all, for the reasons above stated, was not proportionately large. I have elsewhere<sup>3</sup> given detailed figures on the point for one year. Other years presented much the same sort of facts. The important feature is that these irremediable losses, so far as all the evidence indicates, have been random samples of the population in respect of sex. Further on in the paper detailed evidence in support of this statement will be presented.

In the statistical treatment of the data the mating or family has been made the unit, wherever such treatment is possible. While not novel, this method of dealing with sex ratio statistics is unusual. It has certain marked advantages, from a methodological viewpoint, over the more usual procedure of considering a whole population as the unit in studying the sex ratio. These advantages will be apparent as we proceed.

Throughout this paper the sex ratio is presented as the percentage of the males in the total of the group or population. Or, in other words, we express the sex ratio as

<sup>3</sup> Pearl, R., *Amer. Nat.*, Vol. XLV., pp. 107-117. 1911.

$$R_{\sigma} = \frac{100 \sigma^1 \sigma^2}{\sigma^1 \sigma^1 + \sigma^1 \sigma^2}$$

for any mating, group or population. To convert any such sex ratio into the form where the proportion of the sexes is expressed as number of males per 100 females one has only to divide the given  $R$  by  $100-R$ , and the answer, multiplied by 100, will be the result sought.

### III. THE NORMAL SEX RATIO IN THE FOWL.

In dealing with sex ratios with the single mating or family as the unit it is evident that the absolute size of the family from each mating is a factor which must be considered. In a family of 2 the only possible values for  $R_{\sigma}$  are 0, 50, and 100 per cent. Again, a single family of 2 is a very small sample of the gametic population of the parents. The larger the family, obviously, the better the sampling. Now in the usual method of dealing statistically with sex ratios, where one simply counts all the males and all the females in the population, no account whatever is taken nor can be, of the badness of the gametic sampling in case of very small families. A male in a family of 1 counts as significantly toward the final result as a male in a family of 30. Yet it is quite sure that if we determined the sex ratio of the population on the basis of families of 1 only, the result would be less worthy of confidence (*i. e.*, of a larger "probable" error) than if it were based on large families only.

Tables I. to III. inclusive give the distribution to the sex ratios for all fertile matings of the domestic fowl made by the writer in the eight years from 1908 to 1915 inclusive. Sterile matings are, of course, not included. The data are divided between the three tables on the basis of size of family. Table I. includes only families in which 10 or more chicks were produced. Table II. includes families of from 4 to 9 chicks, and Table III. covers the very small families of 1, 2, or 3 chicks only. In order that there may be no misunderstanding it will be well to state clearly just the significance of these tables. To take an example: The entry 2 in the first row of Table I. means that in the year 1908 there were produced 2 families, each containing 10 or more chicks, in each of which families

# PEARL—SEX RATIO IN DOMESTIC FOWL.

## TABLE I.

FREQUENCY DISTRIBUTION OF THE SEX RATIO IN THE FOWL. VARIOUS BREEDS.  
FAMILIES OF 10 AND OVER.

Year.	Sex Ratio $R_{\sigma}^{\gamma}$ .									
	0-9.9.	10.0-19.9.	20.0-29.9.	30.0-39.9.	40.0-49.9.	50.0-59.9.	60.0-69.9.	70.0-79.9.	80.0-89.9.	90.0-100.0.
1908 .....	0	2	10	22	22	18	11	7	0	1
1909 .....	0	4	9	17	38	46	29	2	0	0
1910 .....	0	3	14	17	36	52	24	11	3	1
1911 .....	1	3	12	34	43	37	18	11	2	1
1912 .....	0	3	6	19	36	41	22	8	2	0
1913 .....	0	1	6	16	23	33	17	6	0	0
1914 .....	0	2	2	17	35	48	27	1	0	0
1915 .....	0	1	4	16	27	19	12	1	0	0
Totals..	1	19	60	158	260	294	160	47	7	3

## TABLE II.

FREQUENCY DISTRIBUTION OF THE SEX RATIO IN THE FOWL. VARIOUS BREEDS.  
FAMILIES OF 4 TO 9 INCLUSIVE.

Year.	Sex Ratio. $R_{\sigma}^{\gamma}$ .									
	0-9.9.	10.0-19.9.	20.0-29.9.	30.0-39.9.	40.0-49.9.	50.0-59.9.	60.0-69.9.	70.0-79.9.	80.0-89.9.	90.0-100.0.
1908 .....	5	4	12	7	9	17	13	6	1	3
1909 .....	0	2	9	6	3	14	7	2	2	2
1910 .....	3	2	7	2	1	6	4	2	0	7
1911 .....	9	1	3	8	5	12	8	5	3	4
1912 .....	1	1	3	5	5	8	5	8	3	2
1913 .....	0	2	4	3	6	12	10	5	1	2
1914 .....	2	0	6	6	3	5	4	3	4	2
1915 .....	2	1	2	2	7	13	5	4	3	1
Totals..	22	13	46	39	39	87	56	35	17	23

## TABLE III.

FREQUENCY DISTRIBUTION OF THE SEX RATIO IN THE FOWL. VARIOUS BREEDS.  
FAMILIES OF 1 TO 3 INCLUSIVE.

Year.	Sex Ratio. $R_{\sigma}^{\gamma}$ .									
	0-9.9.	10.0-19.9.	20.0-29.9.	30.0-39.9.	40.0-49.9.	50.0-59.9.	60.0-69.9.	70.0-79.9.	80.0-89.9.	90.0-100.0.
1908 .....	8	0	0	1	0	6	4	0	0	11
1909 .....	6	0	0	2	0	3	4	0	0	4
1910 .....	5	0	0	0	0	2	2	0	0	2
1911 .....	10	0	0	0	0	6	1	0	0	11
1912 .....	2	0	0	2	0	5	2	0	0	8
1913 .....	2	0	0	2	0	6	4	0	0	9
1914 .....	6	0	0	1	0	2	0	0	0	9
1915 .....	4	0	0	3	0	2	1	0	0	3
Totals..	43	0	0	11	0	32	18	0	0	57

the percentage of ♂♂ to total number of chicks was somewhere between 10 per cent. and 19.9 per cent. Other entries are to be correspondingly read.

The first thing which strikes one's attention in examining these tables is that extreme values of the sex ratio (below 20 and above 80 say) occur relatively frequently only in small families. If the families are very small (Table III.) extreme values of the sex ratio become actually *more* frequent than medium values. The greater frequency of extreme sex ratios in small families is obviously what would be expected on merely arithmetic grounds. Thus to take the data of Table III. We find from the original records that there were 54 families of 1, 53 of 2, and 54 of 3 each contributing to this table. Suppose males and females were equally likely to occur (*i. e.*,  $R_{\sigma}=50$ ); then according to the laws of chance, the totals of Table III. would be expected to be as shown in Table IV. These are compared with the actually observed totals.

TABLE IV.

COMPARING TOTALS OF TABLE III., WITH CHANCE DISTRIBUTION OF SAME NUMBER OF FAMILIES, ON THE ASSUMPTION THAT  $R_{\sigma}=50$ .

Distribution.	Sex Ratio.									
	0-9.9.	10.0-19.9.	20.0-29.9.	30.0-39.9.	40.0-49.9.	50.0-59.9.	60.0-69.9.	70.0-79.9.	80.0-89.9.	90.0-100.0.
Actual . . . .	43	0	0	11	0	32	18	0	0	57
Chance . . . .	47	0	0	20.25	0	26.50	20.25	0	0	47

While this is by no means a perfect fit of the observations by the chance distribution, the latter is close enough to the former to indicate clearly the essentially chance determined character of the observed distribution. The resemblance would be still closer if we took a value of  $R_{\sigma}$  for the computation more nearly in accord with the actual fact than is 50, the value actually used.

There is no need to pursue this point further, as it will be evident to anyone who will examine Tables I., II. and III., in the light of the points just made, that we cannot draw any conclusions of critical value regarding the normal *variation* of the sex ratio in the fowl, at least, except on the basis of families containing at least 10 individuals each.

We may next consider the mean sex ratio, dealing separately with each of the three groups. In calculating these means, and the other variation constants, it was not assumed, as is ordinarily done, that each class centered at the mid-point of the strip of base on which its frequency stands. To have done so would have involved a considerable error. Instead the actual centering point for each class was determined from the individual records. The results are shown in Table V., and from this table one can see how large the error involved in the usual statistical assumption would have been. The reason for the error is, of course, purely arithmetical, and arises from the fact that in small groups, such as the families here dealt with, only certain percentage values are possible.

Using the values of Table V., we get, by ordinary methods, the

TABLE V.

SHOWING THE ACTUAL CENTERING POINTS OF THE SEVERAL CLASSES IN TABLES I., II., AND III.

Class.	Centering Point.		
	Families 10 and Over.	Families 4-9 Inclusive.	Families 1-3 Inclusive.
0 - 9.9 .....	0	0	0
10.0- 19.9 .....	15.46	15.68	—
20.0- 29.9 .....	24.87	24.15	—
30.0- 39.9 .....	35.01	33.70	33.33
40.0- 49.9 .....	43.97	41.73	—
50.0- 59.9 .....	53.57	51.76	50.00
60.0- 69.9 .....	63.65	63.51	66.67
70.0- 79.9 .....	73.09	74.58	—
80.0- 89.9 .....	84.64	81.92	—
90.0-100.0 .....	100.00	100.00	100.00

mean sex ratios exhibited in Table VI. We shall deal at this point only with the total distribution of Tables I., II., and III.

TABLE VI.

MEAN SEX RATIO OF THE DOMESTIC FOWL. VARIOUS BREEDS.

Group.	$R_{\sigma}$ .	$\sigma^{\sigma}$ per 1,000 $\sigma^{\sigma}$
Families of 10 and over (Total Table I.) .....	$48.57 \pm 0.28$	944
Families of 4 to 9 inclusive (Total Table II.) .....	$49.39 \pm .84$	976
Families of 1 to 3 inclusive (Total Table III.) .....	$55.07 \pm 2.11$	1226
Families of 4 and over (Tables I. and II. combined)	$48.80 \pm .33$	953
Families of all sizes (Tables I., II., and III. combined)	49.45	978



These figures show that if we take all of the 22,791 chicks, on which this table is based, into account together we get a mean sex ratio of 49.45, or approximately one half of one per cent. fewer males than females. This, however, cannot be regarded as the normal sex ratio for the strains of poultry and the environmental complex here dealt with, because (a) the table shows an obvious influence of size of family on the sex ratio, a point to which we shall return for detailed discussion later in the paper, and (b) families under 10 cannot be considered as representative of the normal fertility of the *domestic* fowl. The value for families of 10 and over, namely  $R_{\sigma} = 48.58 \pm .28$  (944), is certainly to be regarded as much nearer the true biological norm for the sex ratio of this group of poultry under the environmental conditions prevailing at the Maine Station.

Taking this value as the normal one, how does it compare with other values for other strains of poultry, and for other birds domestic and wild? Unfortunately there are very few data available for comparison. Curiously enough, this lack is most pronounced where it would be least expected,—namely in the case of poultry. Table VII. contains all the data, involving numbers large enough to be statistically of any significance, with which the writer is ac-

TABLE VII.  
SEX RATIO STATISTICS FOR VARIOUS BIRDS.

Bird.	Total No.	$R_{\sigma}$ .	$\sigma^{\circ} \sigma^{\circ}$ per 1,000 $\phi^{\circ} \phi^{\circ}$ .	Authority.
Pigeon.....	136	53.68	1,159	Cuénot <sup>4</sup>
Pigeon.....	1,648	51.27	1,052	Cole and Kirkpatrick <sup>5</sup>
Canary.....	200	43.52	770	Heape <sup>6</sup>
Canary.....	68	77.94	3,533	Heape <sup>6</sup>
Fowl.....	1,001	48.64	947	Darwin <sup>7</sup>
Fowl.....	2,105	44.63	806	Field <sup>8</sup>
Fowl.....	20,037	48.57	944	Pearl, this paper. Families of 10 and over.

<sup>4</sup> Cuénot, L., *Bulletin Sci. France et Belg.*, T. 32 (5th Ser., T. 1), pp. 462-535, 1899.

<sup>5</sup> Cole, L. J., and Kirkpatrick, W. F., *Rhode Island Agric. Expt. Stat. Bulletin*, 162, pp. 463-512. 1915.

<sup>6</sup> Heape, W., *Proc. Cambridge Phil. Soc.*, Vol. XIV., pp. 201-205, 1907.

<sup>7</sup> Darwin, C., "The Descent of Man," Vol. I.

<sup>8</sup> Field, G. W., *Biol. Bulletin*, Vol. II., pp. 360-361, 1901.

quainted. If, as may well be the case, he has overlooked some extensive tabulations of sex ratios in birds, he will be very grateful for the pertinent references.

It is evident enough from these figures that the sex ratio varies in domestic birds quite as extensively as it does among domestic mammals. In general there would appear to be a tendency toward the production of a slight excess of males among two of the sorts of birds here dealt with. This seems certainly true for pigeons. The canary results are not very clear either way. Heape gives data on the sexes from two canary breeders. The results are widely different. This difference in sex ratios Heape attributes to differences in the mode of managing the breeding birds. Here it suffices merely to point out that in any case, the numbers on which the canary ratios are based are statistically very small. It may well be doubted whether the deviations exhibited in Heape's material are in reality significant.

In the fowl the case appears to be different, all available statistics agreeing in showing a normal excess of females. It is, however, the opinion of many poultrymen of long experience, that the usual condition is practical equality of the sexes, with a small but steady preponderance of males—a sort of sex ratio similar to that which man exhibits. The practical equality of the production of the sexes in poultry has been noted by various writers.<sup>9</sup>

But all of the actual statistics which I have been able to find show the slight preponderance to be of females and not of males. The agreement between Darwin's figures and those of the present investigation is nearly perfect. General experience of poultrymen would indicate that the very low sex ratio got by Field could not be considered as normally representative of fowls in general. The close agreement of my figures with Darwin's, collected rather more than a decade later than Field's, would seem definitely to negative the suggestion of the latter that the normal proportion of the sexes in poultry has actually changed since Darwin's time "as a result of the breeders' desire to produce a larger proportion of females."

<sup>9</sup> E. g., Beeck, A., "Die Federviehzucht," Bd. I., Berlin, 1908, p. 563.

Lewis, H. R., "Productive Poultry Husbandry," Philadelphia, 1913, p. 250.

It is to be regretted that more of those who have used poultry as experimental material have not kept and published accurate and complete figures of sex production.

In any case the immediate problem before us is clearly to attempt by analysis of the figures to learn what influence various factors may have in the production of the excess of females plainly shown in the extensive statistics of the present paper. The chromosomal mechanism of sex determination in the individual case would lead us to expect an equality of the sexes in statistically large numbers. But it is plain that, even with very large numbers, no such equality is attained. There must be reasons, scientifically ascertainable, for this deviation. It is our problem to find what these reasons are.

In undertaking such analysis let us first see whether the excess production of females is a secularly regular phenomenon in this stock and under our conditions.

The mean sex ratios for each year for families of 10 and over are set forth in Table VIII.

TABLE VIII.

SHOWING THE YEARLY CHANGES IN MEAN SEX RATIO. FAMILIES OF 10 AND OVER.

Year.	Mean $R_{\sigma}$ .
1908 .....	46.16 $\pm$ 1.07
1909 .....	48.33 $\pm$ .69
1910 .....	49.96 $\pm$ .78
1911 .....	47.08 $\pm$ .79
1912 .....	49.59 $\pm$ .77
1913 .....	49.999 $\pm$ .81
1914 .....	49.83 $\pm$ .62
1915 .....	46.46 $\pm$ .86

The data of this table are shown graphically in Fig. 1.

From the table and diagram it is evident that the excess of females is not a sporadic, but rather a regular phenomenon in our stock and conditions. While at times the ratio comes very close to 50 (*e. g.*, in 1913) it never quite reaches that value. The fluctuations of the ratio in successive years appear to be entirely random.

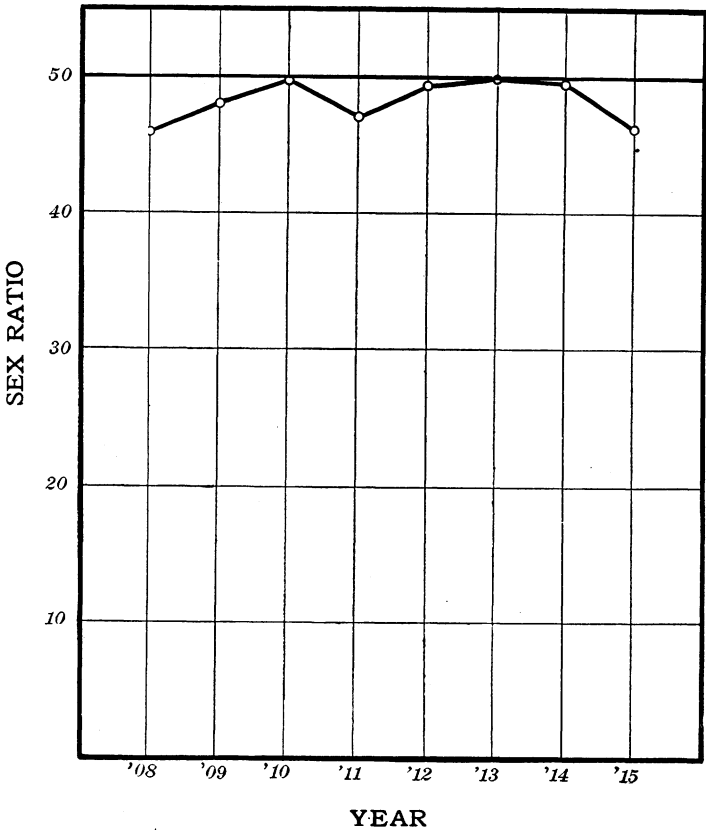


FIG. 1. Showing the mean ♂ sex ratio in consecutive years.

III. THE NORMAL VARIATION OF THE SEX RATIO.

So far we have considered only mean values. Let us now examine the dispersion or variation constants. From the totals of Tables I., II., and III. we deduce the standard deviations set forth in Table IX, by the ordinary method.

TABLE IX.

STANDARD DEVIATION OF THE SEX RATIO OF THE DOMESTIC FOWL. VARIOUS BREEDS.

Group.	$R_{\sigma}.$	
Families of 10 and over .....	13.37 $\pm$	.20
Families of 4 to 9 inclusive .....	24.18 $\pm$	.59
Families of 1 to 3 inclusive .....	39.72 $\pm$	1.49
Families of 4 and over .....	18.30 $\pm$	.23

The striking fact which this table brings out is the great reduction in the variation of the sex ratio from mating to mating as the progeny from the individual mating becomes more numerous.

Even with the large families, however, the amount of variation in the sex ratio is large, absolutely and relatively. Taking families of 10 the percentage of the standard deviation in the mean is 27.53.

This is of roughly the same order of magnitude as the coefficients of variation of such physiological characters as fecundity,<sup>10</sup> etc. There can be no question that the sex ratio is relatively a much more variable character than stature, skull form, and most other morphological characters of animals. In view of this fact, there would seem to be need of vastly more caution than is commonly exercised by writers on the sex ratio in drawing far-reaching conclusions from very small numbers.

The values for the standard deviation of the sex-ratio here obtained for poultry are of the same general order of magnitude as those of Heron<sup>11</sup> for man and horse, and of Weldon<sup>12</sup> for mice.

The form of the normal sex-ratio variation curve is of interest. In order to deal with this adequately, we must resort to the analytical methods of Pearson.<sup>13</sup>

The case presents some difficulties from the standpoint of graphical representation, because of the fact pointed out above, that we have dealt with the actual centers of gravity of each piece of area standing over a unit on the abscissal axis, and have not assumed as is usually done, that the center of gravity of each strip was at its mid-point. The conventional histogram does not give any representation of this distorted concentration, and hence the correct fitted curve does not *seem* to give so true a representation of the facts as an incorrect one, as will presently appear.

<sup>10</sup> Cf. Pearl, R., *Science*, Vol. 37, p. 228, 1913.

<sup>11</sup> Heron, D., *Biometrika*, Vol. V., pp. 79-85, 1906.

<sup>12</sup> "On Heredity in Mice from the Records of the Late W. F. R. Weldon. Part I. On the Inheritance of the Sex-ratio and of the Size of Litter," *Biometrika*, Vol. V., pp. 437-449, 1907.

<sup>13</sup> Pearson, K., *Phil. Trans.*, Vol. 86 A, pp. 343-414, 10 pls., 1895, *ibid.*, Vol. 197 A, pp. 443-459, 1901.

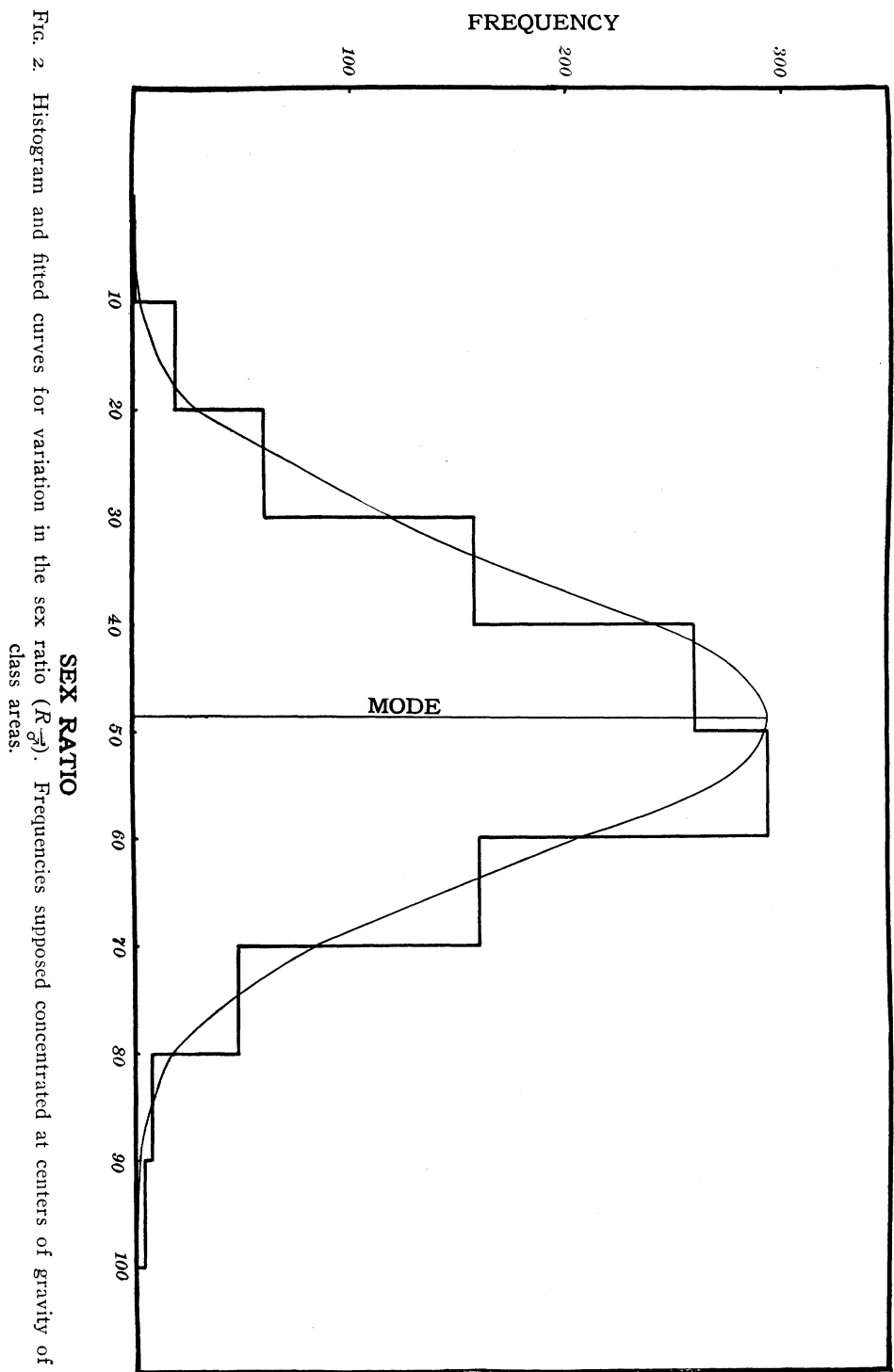
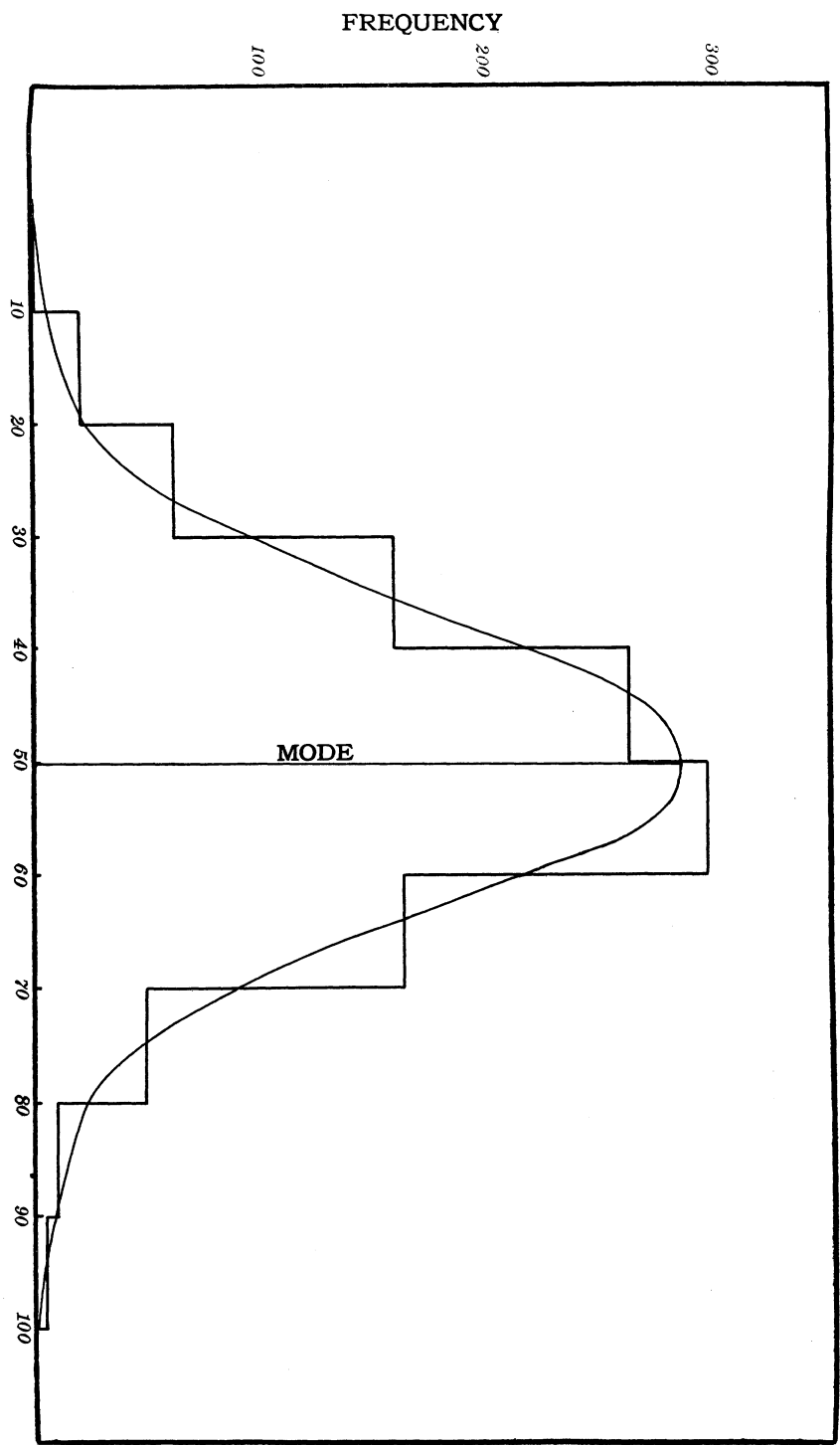


Fig. 3. Histogram and fitted curve for variation in the sex ratio ( $R_g$ ). Frequencies supposed concentrated at mid-points of class areas.



In Table X. are given the true analytical constants of the curve, and, in another column, the analytical constants on the assumption of concentration of the frequencies at the mid-points of the classes.

TABLE X.  
ANALYTICAL CONSTANTS FOR VARIATION IN THE SEX RATE IN POULTRY,  
VARIOUS BREEDS. FAMILIES OF 10 AND OVER.

Constant.	Frequencies Supposed Concentrated at Centers of Gravity of Class Areas.	Frequencies Supposed Concentrated at Mid-points of Class Areas.
$N$ .....	1009	1009
Mean .....	48.574	49.549
$\mu_2$ .....	1.7887	1.8197
$\mu_3$ .....	— .0093	— .2622
$\mu_4$ .....	11.0082	10.2718
$\beta_1$ .....	.000015	.0114
$\beta_2$ .....	3.4407	3.1019
$\kappa_1$ .....	+ .8814	+ .1696
$\kappa_2$ .....	+ .000013	+ .0506
Type .....	VII.	IV.
Mode .....	48.574	50.231
Skewness .....	— .0015 $\pm$ .0264	— .0506 $\pm$ .0027
$y_0$ .....	315.25	42.740

The equations to these curves are as follows:

True curve:

$$y = 315.25 \left( 1 + \frac{x^2}{27.9292} \right)^{-9.3072},$$

Mid-point curve:

$$y = 42.740 \frac{e^{-17.0718 \tan^{-1} (x/11.2271)}}{\left( 1 + \frac{x^2}{126.0478} \right)^{37.9784}}.$$

The fitted curves and the histograms are shown in Figs. 2 and 3.

From the data and the diagrams, the following points are to be noted:

1. The distribution of the sex ratio about the mean value is approximately symmetrical, and, if sufficiently large families are used, leads to high contact of the curves at both ends of the range.

2. The distribution is *apparently* more skew than it actually is because of the fact that this graphical representation makes no ac-



count of the concentration of the frequency at other than the mid-points of the class areas.

3. The fitted curve makes it possible to make some rather definite statements as to the probability of the occurrence, as a result of chance merely, of distinctly aberrant sex ratios. Poultry papers very frequently, and scientific journals rather more often than would seem compatible with any clear grasp of the theory of chance, contain statements about marvelous deviations from the normal sex ratio in particular families or small groups of families. Usually such widely divergent sex ratios are most uncritically taken to prove either the inheritance of a special sex tendency in a particular line of breeding, or the influence of some external environmental agent upon sex determination. If, for example, a poultry breeder finds that out of twenty chickens from one pair of parents, fifteen are pullets, he is distinctly apt to regard this as a wonderful phenomenon, worthy of his best exegetic powers. But our present statistics show that, if we deal with families of twenty chickens for example, it is to be expected on the basis of chance alone, the following relations will hold.

15 or more chicks will be pullets in	56 out of every 1,000 families of 20
16 or more chicks will be pullets in	26 out of every 1,000 families of 20
17 or more chicks will be pullets in	12 out of every 1,000 families of 20
18 or more chicks will be pullets in	5 out of every 1,000 families of 20
19 or more chicks will be pullets in	2 out of every 1,000 families of 20
20 or more chicks will be pullets in	1 out of every 1,000 families of 20

It needs no particular emphasis on these figures to indicate that before aberrant sex ratios can be considered indicative either of environmental or hereditary effects, it will be necessary to show that they occur with such frequency as to exceed considerably that expected on the basis of chance alone.

#### IV. PRENATAL MORTALITY AND THE SEX RATIO.

The first suggestion which comes into one's mind in attempting any analysis of the causes of a deviation of the sex ratio from equality, is that the prenatal mortality has been differential in respect to sex. It is commonly held by statistical writers that this is true of

some portion, at least, of the prenatal mortality in man. In still births there is a greater excess of males over females than in living births. The reviews which prevail among statistical writers regarding this matter are well put by Nichols<sup>14</sup> (p. 269) in the following passage:

"Obviously the main cause of the great preponderance of male stillbirths resolves itself into the question of the comparative mortality or death rate of the male and female sexes during the intrauterine period of existence. Vital statistics have shown clearly that there are material differences in the mortality of the two sexes, the death rates among males being, in general, higher than among females throughout nearly the entire period of life, and the average duration of life of females being greater than of males. During the adult and later periods of life this difference is largely or partly explainable on the ground of the greater stress and strain and liability to injury imposed by the greater responsibilities, more laborious occupations, and greater exposure of men, and their greater indulgence in vicious and morbid habits; these factors scarcely being offset by the perils incurred by women during the child-bearing period. But the same greater mortality of males occurs, and in the most marked degree, even in the intrauterine period of existence and in the early years of life before the factors mentioned begin to be operative; it is therefore obvious that the male constitution is intrinsically weaker, less hardy, and more susceptible to morbid and mortific influences, and has less vitality and resisting power against disease, than the female. The cause of this innate disparity of vitality between the two sexes we do not know; but the fact it exists, that the antenatal mortality and death rate of males much exceeds that of female fetuses, accounts for the great excess of male over female stillbirths."

The demographic objects, in the study of sex ratios, are somewhat different than the purely biological. In the present instance, and generally in purely biological studies on the proportion of the sexes, what we really wish to know is the true proportions in which *zygotes* of the two sexes are *initially* produced. This can not be directly observed in higher vertebrates, owing to the occurrence of prenatal mortality at all stages between the fertilization of the egg and the birth of the young. The earliest easily observable datum plane which one has upon which to base a conclusion as to the sex proportions in the *zygotes* at the moment of their production, is the sex ratio at birth. Obviously the prenatal mortality may have influenced

<sup>14</sup> Nichols, J. B., *Mem. Amer. Anthropol. Assoc.*, Vol. I., Part 4, pp. 249-300, 1907.

this ratio, and caused a deviation from the initial zygotic ratio. But it is equally obvious that the post-natal mortality, whether differential in respect of sex or not, can give us no direct aid in estimating the initial zygotic ratio from the observed ratio at birth. Hence the post-natal mortality has no special interest in connection with sex studies to the biologist, though it does have to the demographer, who is concerned, among other things, with the sex distribution of populations throughout life.

In poultry, the hatched chicks show a certain fairly definite ratio of males to females as we have seen. Does this observed ratio at birth differ from the initial zygotic sex ratio? To answer this question, it is only necessary to determine whether the sex ratio of the zygotes which die before hatching is, or is not, different from the sex ratio of those which hatch. Theoretically this should be simple. Practically it is not wholly so. The difficulty is that the sex of the zygote is not distinguishable by any practical means until the embryo reaches a certain more or less advanced stage of development. If zygotes die before that stage of development is reached, as some do, then it becomes impossible practically to determine whether that particular moiety of the mortality was or was not differential in respect to sex. Theoretically, of course, one should be able to sex every zygote by means of a cytological examination of its chromosomes. Practically, however, this is not to be seriously considered.

The result is that in the chick it is practically impossible to say absolutely whether the mortality between the fertilization of the egg and about the tenth day of development of the embryo is or is not differential. We can, however, determine, with great precision, the facts regarding the mortality from the tenth day to the end of incubation. This has been done by the writer, during the past two years. Every egg in which the embryo developed to the tenth day or beyond, and died before hatching, has been opened, the embryo removed and dissected, and its sex and certain other characteristics recorded. This is distinctly tedious and unpleasant work, but there appears to be no alternative method of getting certain sorts of information very essential in the analysis of many problems.

The figures for the sex ratio of the dead embryos for the years 1916 and 1917, the only ones for which complete records are at hand, are given in Table XI.

TABLE XI.

SEX RATIO OF EMBRYOS DYING BETWEEN THE TENTH DAY OF INCUBATION AND HATCHING. VARIOUS BREEDS.

Year.	♂♂.	♀♀.	$R_{\sigma}$ .
1916.....	325	343	$48.7 \pm 1.30$
1917.....	602	651	$48.0 \pm .95$
Totals.....	927	994	$48.3 \pm .77$

These numbers are large enough so that the results are clearly reliable. And it is equally clear that this portion of the prenatal mortality is not differential in respect to sex. For the season of 1916 the sex ratio of the living chicks at hatching was

$$R_{\sigma} = 48.3 \pm 0.89,$$

a value not significantly different from that for the prenatal mortality given in Table XI. The sex-ratio figures for the living hatched in 1917 are not available at the time of writing, but it is evident enough, if we compare the figures of Table XI. with those of Table VI. (p. 422), that there is no differentiation in respect of sex of the mortality of the last eleven days of the prenatal life of the zygote.

Cole and Kirkpatrick's<sup>5</sup> data for pigeons appear to indicate that probably the prenatal mortality in that form is not differential. It must be said, however, that they take account of only a small amount of the total prenatal mortality, those dying at the very end of incubation, then group this with the *post*-natal mortality of the first five days after hatching. The general impression given by this data, however, is that the prenatal mortality is probably not differential in the pigeon.

It is evident from the data of Table XI., that the explanation for the preponderance of females in poultry is not to be found in the greater frequency of deaths of males during the last eleven

days of incubation. But there remains a certain mortality during the first ten days. We are in position to say, on the basis of evidence already given, that in the Maine Station flock male and female zygotes are present in the proportion indicated by  $R_{\sigma} = 48.5$  at the time when the zygotes are 10 days old. Were they initially present in equal numbers and did enough more males than females die during the period to the tenth day of incubation to produce the  $R_{\sigma} = 48.5$  status? Here we would call attention only to two points. The first is that in the flocks which have furnished the statistics here dealt with, the rate of prenatal mortality before the tenth day of incubation has always been low—so low that if differential mortality within this period is to be adduced as the explanation of the observed sex ratio, it would be necessary to assume that practically *every* embryo which died within these first ten days was male. A theory can only be regarded as highly improbable which demands that during any period of life all naturally occurring deaths are of individuals of the same sex, when it is known to be the fact that in all other periods of life the individuals of the two sexes die in numbers roughly proportional to the numbers living of each sex.

In the second place, it is in the highest degree improbable that there is an abrupt change in the mode of incidence of the mortality with respect to sex at exactly the tenth day of incubation. Yet such an abrupt change would be demanded by any theory which makes differential mortality the explanation of the observed sex ratio in the fowl. From the time when the embryo has developed sufficiently to make it possible certainly to distinguish the sexes in poultry by macroscopic examination of the gonads, we know that the mortality is either not differential at all with respect to sex (prenatal mortality), or is at most only slightly so (possibly so in postnatal mortality though the point has not been fully investigated yet). In the absence of any evidence favorable to such a view, it could only be regarded as a highly improbable speculation to say that in the very earliest stages of embryonic development all deaths are males.

We are justified, I think, in concluding that in the flocks of poultry here dealt with, and probably in the fowl generally, that

*prenatal mortality is not differential in respect to sex, and that in consequence the observed sex ratio at birth is substantially the same as the initial zygotic sex ratio.*

#### V. CONCLUSION.

The purpose of this paper is to present data regarding the normal sex ratio in the domestic fowl. The data involves something over 22,000 chicks. The normal variability in sex ratio is discussed. It is hoped in a later paper to present a further analysis of the subject dealing with the influence of various internal and external factors upon the sex ratio. It was expected to include such discussion in the present paper but for reasons explained at the beginning of the paper this is not now possible.